## LETTERS TO THE EDITOR.

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### The Origin of Radium.

I CANNOT let Prof. Rutherford's letter in NATURE of June 6 pass without directing attention to one striking consequence, in which I personally am interested. During 1904 and 1905 I published (NATURE, May 12, 1904, January 26, 1905, and *Phil. Mag.*, June, 1905, p. 768) the result of an experiment which went to show that a kilogram of uranyl nitrate, purified initially from radium by precipitating barium as sulphate in its solution, and kept 550 days, generated a quantity of radium which, although only one-thousandth part of what is theoretically to be expected on the view that a direct change of uranium X into radium takes place, was still one hundred times the amount initially present. Boltwood (*Am. Journ. Sci.*, September, 1905, xx., 239), working with one hundred grams of uranyl nitrate purified from radium initially by repeated crystallisations from water, was unable to observe any detectable increase after a period of 390 days, and concluded that "the results obtained by Mr. Soddy are without significance," and averred that my results were due to the introduction of radium salts during the tests.

Now such a criticism and such an imputation on the part of one investigator dealing with the work of another surely ought only to have been made if it was the only possible explanation of the discrepancy. As it was, to me at least, it was not even the most obvious explanation. Boltwood did not give consideration to the all-important influence of the method of purification of the uranium from radium on the results obtained. My result, that the raté of production of radium from uranium was only one-thousandth of the theoretical, brought into being the present theory of the existence of several hypothetical intermediate transition forms between uranium and radium. It is obvious that, according as the method of purification employed does not or does remove these transition forms as well as the radium, so one will or will not expect to observe an initial production of radium in a solution of uranium. Now the method of precipitating barium as sulphate in a uranium solution is designed to *remove only* the radium, whereas the method of repeated crystallisation from water adopted by Boltwood is well calculated to *purify* the uranium, that is, to free it from all other accompanying substances. Hence there is no necessary discrepancy between the results of the two experiments. This view has been put forward by Rutherford ("Radio-active Transformations," p. 159).

I would not now have raised this matter had not history apparently repeated itself, and Prof. Rutherford's most recent results (NATURE, June 6, p. 126) enabled me, without making any special claim to infallibility, to exhibit clearly the real nature of Boltwood's criticism. In the *American Journal of Science* for December, 1906, p. 537 (NATURE, January 3), Boltwood published a "Note on the production of Radium from Actinium" in which evidence was given that actinium is the parent of radium. This was quickly followed (NATURE, January 17, p. 270) by some confirmatory evidence of a similar character by Rutherford, who, however, pointed out that there was no proof that actinium was itself the true parent of radium, although this parent was undoubtedly present in the actinium solutions employed. Now Rutherford shows in last week's issue that actinium *purified from radium in a different manner* yields no appreciable growth of radium. Is Boltwood's previous positive result then "without significance"? Surely not. But if Boltwood's result on the production of radium from actinium can be explained, as, of course, it can be explained, without charging him with introducing radium into his solution, so in the same way can mine with uranium. Indeed, whereas the intermediate product, which is the parent of radium, is a *necessary* companion of *any* uranium preparation which has not recently been subjected to a purification process capable of removing it, it has yet to be shown that the association of this parent with actinium is genetic and not purely fortuitous.

I hope this exposure of an old criticism, made without due consideration of the complexity of the problem, will clear the way for the publication of some further results. In the two years that have elapsed since the publication of my last paper I have had the advantage of the co-operation of Mr. T. D. Mackenzie in the steady and continuous prosecution of the work under the most favourable conditions. We have from the commencement, which dates prior to Boltwood's first communication on the subject, had as the basis of the work the all-important influence of the method of purification adopted, and we have used throughout a new method of purification, which, though not without difficulty and danger in its application to the purification of large quantities of uranium salts, was deliberately chosen as affording a reasonable guarantee that it would separate the uranium from all other substances present. Mr. Mackenzie has purified with the utmost care three separate kilograms of uranyl nitrate by this method, and I may anticipate our results to the extent of saying that, so far, they entirely confirm and extend the results obtained by Boltwood in which re-crystallisation was the method of purification employed. The first preparation, containing after purification about 500 grams of uranyl nitrate, has been kept for 600 days, and has not shown the slightest detectable increase in the amount of radium initially present. Now that these three purified preparations have been set up in a form to allow of continuous and extended observation, our attention is being directed to the residues from the three kilograms, which should contain the parent of radium, if my earlier positive result was correct. After all, it would be a little surprising if this parent of radium was entirely absent from commercial salts of uranium, for although Boltwood and Rutherford have found it in preparations of actinium, it must not be forgotten that the only source of actinium is that from which commercial uranium salts are prepared.

FREDERICK SODDY.

The University, Glasgow, June 8.

### The Structure of the Æther.

IN the current number of the *Philosophical Magazine* I have given in some detail certain objections to identifying the magnetic vector with translational æthereal motion, and to a large extent these are on all fours with Prof. Hicks's objection, which is cited by Sir Oliver Lodge in the same number, and of which I had lost sight. Very briefly, thus: if bodily æther flow were (within a constant factor) identical with magnetic induction, or were even an essential feature thereof, our judgment as to whether or not a given region was pervaded by magnetic induction would depend on the arbitrary origin of coordinate axes relatively to which we chose to measure velocities, motion of bodies through the æther being physically indistinguishable from an equal and opposite motion of the æther with those bodies at rest.

Much the same difficulty (concerning the essential relativity of motion) seems to me to arise when resultant æthereal momentum is taken to correspond to the vector product of the electric and magnetic vectors; in this case, moreover, further difficulties are encountered. Consideration of a progressive train of electromagnetic waves shows that, with this æthereal-flow interpretation of the Poynting vector, we should have a resultant æthereal motion made up of a steady flow in the direction of wave propagation, together with to-and-fro motions parallel to that direction and kinematically exactly simulating the motion of a gas which is transmitting waves of sound. This clearly implies *compressibility of the aether*, not merely as a minute residual phenomenon, but as a fundamental relation of electromagnetism.

And what would happen in the case of such a body as the sun, which consistently radiates more energy than it receives by radiation? There would be a flow of æther outward in all directions, maintained throughout immense periods of time. This difficulty seems almost insuperable.

There appears to me to be much evidence in favour of the view that the resultant velocity of the æther (referred

to suitable axes) is everywhere zero—at least so far as electromagnetic phenomena are concerned.

Though I find myself in agreement with Prof. Richardson's conclusion that magnetic intensity is not to be identified with speed of æthereal flow, as explained in his letter to NATURE of May 23, I venture to dissent from his arguments. These proceed from the contention that, on the contested assumption, certain integrals would become infinite. Now, in the first place, it appears to me from mere inspection that both these integrals (which I have not actually evaluated) are in reality finite; in the second place, neither integral expresses a magnitude which bears directly on the point at issue, one of them being justly criticised by Sir Oliver Lodge in NATURE of June 6 as apparently devoid of mathematical meaning. The question proposed is as to the momentum due to an electric charge upon a moving sphere, and in this connection the really significant magnitude is the kinetic energy, expressed in terms of the translational velocity. Differentiating this expression with respect to the velocity, we have at once the momentum, the result obtained being independent of any physical theory as to the ultimate nature of the energy in a magnetic field.

C. V. BURTON.

Cambridge, June 8.

## Decomposition of Radium Bromide.

YESTERDAY, on opening a glass tube containing 1 milligram of radium bromide which had been hermetically sealed for almost exactly twelve months, there was a very strong odour of bromine which hung about the tube for about ten minutes. The amount of the bromide decomposed in this period would be about $5\cdot4\times10^{-7}$ grams according to Rutherford; the amount of bromine corresponding to this would be about $2\times10^{-7}$ grams. Perhaps some chemist could say definitely whether this amount of bromine would be detectable by its odour. The volume of the tube was about 4 cubic centimetres.

ALFRED W. PORTER.

University College, London, June 8.

## The Mass of the α Particle.

APPARENTLY the following simple and obvious method of calculating the mass of the α particle has been overlooked.

According to Rutherford, the number of α particles emitted per second by a gram of pure radium is $2\cdot5\times10^{11}$. Of these particles, one-quarter comes from each of the four elements Ra, RaEm, RaA, RaC. The particles from these four elements are emitted with velocities $0\cdot82\ V_0$, $0\cdot87\ V_0$, $0\cdot90\ V_0$, $1\cdot00\ V_0$ respectively, where $V_0$ is $2\cdot6\times10^9$ cm./sec.; they all cease to produce ionisation when their velocity is $0\cdot43\ V_0$. Hence the loss of kinetic energy of all the α particles emitted from one gram of radium in passing over their ionising ranges is

$$\frac{2\cdot5\times10^{11}}{4}\times\tfrac{1}{2}m\times\{(0\cdot82)^2+(0\cdot87)^2+(0\cdot90)^2+1^2-4\times(0\cdot43)^2\}(2\cdot6\times10^9)^2=m\times5\cdot3\times10^{29},$$

where $m$ is the mass of an α particle.

At the same time, it is known that one gram of radium gives out 105 gram-calories per hour (mean value), or $1\cdot22\times10^6$ ergs per second. If we may identify this quantity of heat energy with the kinetic energy lost by the α particles in ionisation we have

$$m\times5\cdot3\times10^{29}=1\cdot22\times10^6$$

or

$$m=2\cdot3\times10^{-24}.$$

The ratio $e/m$ for the α particle is $1\cdot56\times10^{14}$ electrostatic units. The two most probable theories of the nature of the α particle are (1) that it consists of an atom of helium carrying a charge $2\epsilon$, where $\epsilon$ is the electronic charge $3\cdot4\times10^{-10}$, and (2) that it is a molecule of hydrogen carrying a charge $\epsilon$. On the hypothesis (1) the mass of the particle is $4\cdot26\times10^{-24}$; on the hypothesis (2) it is $2\cdot13\times10^{-24}$. The calculation given indicates that (2) is correct, and explains the failure of Greinacher and Kernbaum to obtain helium from the α rays of polonium (*Phys. Zeit.*, 1907, p. 339).

If it be assumed that the whole of the kinetic energy of the α particles, and not only that part of it which is spent in ionisation, appears as heat energy, the value for $m$ is found to be

$$1\cdot78\times10^{-24}.$$

I have thought it best to give the maximum estimate of that quantity which can be attained by this method.

NORMAN R. CAMPBELL.

Trinity College, Cambridge, June 3.

## The "Renal-portal System" and Kidney Secretion.

I RECENTLY published a short paper (Proc. Zool. Soc., 1906) on the significance of the so-called "renal-portal system" found in most of the lower Vertebrata. In this paper I advanced strong reasons for supposing that the "renal-portal system," or, as I prefer to call it, renal cardinal meshwork, is non-excretory in nature. I showed that, both developmentally and structurally, there was every reason to doubt whether the renal cardinal meshwork takes any part in the formation of the plexus of blood-vessels which surrounds the urinary tubules (although, of course, these are connected with each other), and that therefore the blood apparently supplied to the kidney by the "renal-portal" (post-renal) vein is in all probability not utilised in the production of the kidney secretion. This conclusion, opposed to that held by most physiologists and morphologists, I supported by citing the physiological experiments of Nussbaum (*Pfluger's Archiv*, xvi., xvii., 1878; *Anat. Anzeig.*, i., 1886) and Beddard (*Jour. Physiol.*, xxviii., 1902), which afforded valuable confirmation. These experiments, as is well known, proved that after the arterial supply of the frog's kidney had been eliminated all secretion immediately stopped, notwithstanding the facts that the "renal-portal" circulation was still in full swing and that powerful diuretics were employed. The sole objection to regarding these experiments as conclusive was that, in consequence of the kidney being deprived of oxygenated blood, the tubular epithelium had degenerated, and was therefore not in a condition to secrete. While recognising this objection, yet for the other reasons which I had already advanced I ventured to maintain that, even if the blood in the post-renal vein could be artificially oxygenated, no secretion would occur.

Unfortunately, I was not aware of more recent physiological work on this subject when I made this last suggestion. Since then, however, Prof. Halliburton has kindly directed my attention to the papers of Bainbridge and Beddard (*Biochemical Journal*, i., 1906) and Cullis (*Jour. Physiol.*, xxxiv., 1906), in which the reverse result has been obtained; that is to say, according to these later experiments, a secretion can be obtained from the "renal-portal" circulation provided that the tubule epithelium is maintained in a healthy condition by means of a sufficient supply of oxygen, and that powerful diuretics like urea and phloridzin are employed. This result at first sight appears to be contradictory of my previous conclusion and confirmatory of the generally accepted "portal" theory of the renal cardinal meshwork, but it is the object of these remarks to show that such is, after all, not necessarily the case.

In the first place, these recent experiments have clearly shown that the "renal-portal" circulation will not yield the slightest secretion in the absence of powerful diuretics; in other words, the result obtained by Bainbridge, Beddard, and Cullis is at best an abnormal one. Under more normal conditions, *i.e.* in the absence of diuretics, with a healthy tubule epithelium and with the "renal-portal system" alone working, no secretion whatever occurs.

Secondly, the very fact that when the venous blood contained in the renal cardinal meshwork alone "supplies" the kidney, the tubule epithelium degenerates, proves that in the normal living animal this blood is not in contact with the tubules, *i.e.* does not take part in the formation of the blood-plexus surrounding the tubules, since, as the experiments prove, these latter require the oxygenated blood derived from the renal arteries in order to live and much more to secrete.

Thirdly, it must be remembered that in the experiments of Bainbridge, Beddard, and Cullis, the elimination of the